# METHODS

FOR

# PROMOTING RESEARCH IN THE EXACT SCIENCES

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# METHODS FOR PROMOTING RESEARCH IN THE EXACT SCIENCES.

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Copies of the following letter of Dr. Simon Newcomb, in which he explains his views of the "method by which the Carnegie Institution can best promote research work in the exact sciences," were sent to several prominent scientific men. A number of the replies which were received follow Dr. Newcomb's letter.

[*Letter of Simon Newcomb.*]

WASHINGTON, D. C., *May 12, 1904.*

The following is a brief summary of views which I have at various times expressed to officers of the Carnegie Institution or made known to the public. They embody my well-matured opinion as to the method by which the Carnegie Institution can most effectively promote research in the exact sciences. I begin by setting forth the main features of the situation.

I.

The nineteenth century has been industriously piling up a vast mass of astronomical, meteorological, magnetical, and sociological observations and data. This accumulation is going on without end and at great expense in every civilized country.

The problem of working out the best results from these observations is one which is not being effectively grappled with. The best methods of attacking the problem are little known to investigators in general, being scarcely developed in a systematic form. The result is that what has been done toward obtaining results consists largely in piecemeal efforts by individuals, frequently leading to no well-established results.

Another feature of the situation is the gradual extension of the principles of exact science into the biological and sociological field.

It is through this extension, rather than through adding to the already accumulated mass of facts, that progress is most to be hoped for in the future.

II.

A consideration which I wish most respectfully to urge upon the Institution is the great advantage which comes from mutual discussion and attrition between men engaged in contiguous fields of work. My own work would have been much more effective could I have enjoyed this advantage more fully, and I am profoundly impressed by the waste of labor shown in an important fraction of current scientific researches through the authors not being acquainted with the best methods of work.

III.

Under these conditions it still seems to me, as it has almost from the day the Institution was founded, that the most effective way in which it can promote research in exact science is by the organization of an institute or bureau of exact science in general. If I had only my special field in view, I might suggest simply an astronomical institute; but it seems to me that this would be too restricted to get the best and most desirable results. I can not but feel it most important that exact methods should be extended into other branches of science than astronomy.

In defining the field of work in such a bureau or institution a division of physical and natural science into three great fields may well be borne in mind. One of these fields is that of the old-fashioned natural science, which is concerned very largely with morphology, physiology, and vital processes which do not admit of reduction to mathematical forms.

Another field is that of purely experimental science.

The third field which really needs development is that of observation, which I propose shall be now occupied. The work required is, in brief, the development of mathematical methods and their application to the great mass of existing observations. Doubtless suggestions as to experiment would frequently come in. These would be carried out by others.

IV.

*The Organization.*—The first requirement for the organization is a managing head in whom the Institution has entire confidence, who should be required to devote all his available energy to the work,

and in doing so should act as the agent of, and be regarded as doing the work of, the Carnegie Institution. He should be supplied with such office, appliances, and assistants as are necessary to commence work in that branch of the field with which he feels himself most conversant, beginning work on a small scale, to be enlarged and extended into neighboring fields as success became assured. The opposite faults of beginning on too large a scale and of making no provision for possible expansion should both be avoided.

V.

The head of the institute should be aided by a council comprising the leading experts best qualified to advise as to the various departments of work. This council might be an international one, and, if the work of the institute is sufficiently expanded to justify it, should hold an annual meeting.

In order to secure the advantages of mutual consultation, attrition, and coöperation, it may eventually be desirable that the work the Institution has already undertaken or is now promoting in the various branches of exact science should be merged with the proposed institute.

VI.

The institute should be started on a very modest scale. The case is one in which everything depends on correct methods from the beginning. By the adoption of these, results may be reached at small expense which, without them, would never be reached with any amount of labor. It seems to me that $10,000 or $15,000 would be ample for the expenses of the first year, as the number of employees who could be successfully put to work would be small. The principal appliances required would be books, but I think that three or four office rooms would suffice for all the purposes of the first year or two.

The expenses of subsequent years would depend upon the expansion which it found desirable to give to the work.

Appended hereto are letters on the subject from Prof. H. H. Turner, of Oxford, and Lord Rayleigh, to each of whom I presented the question of the desirableness of working up the great mass of observations alluded to.

SIMON NEWCOMB.

[*Letter of H. H. Turner.*]

UNIVERSITY OBSERVATORY, OXFORD,
*November 25, 1903.*

I have delayed answering your letter of October 30 for a few days, not from any lack of sympathy with its general purport or doubt as to the value—the immense value—which such a scheme as you suggest would have, but because I wished to think whether I could contribute anything of possible importance to the discussion of details. The result has, however, not been very encouraging, and I must not delay longer a reply on the main point.

I imagine you will not find any one to doubt the necessity of a far more extended discussion of results. In the days of Newton perhaps observations were scarcer than theories, and it was advisable to set them going; but, once set going, inertia has come into play here as elsewhere, and observations of all kinds are churning out masses of observations which no one is attempting to deal with. There is no doubt whatever that it is a crying necessity that we should organize the *discussion* of the masses of accumulated material. The necessity extends beyond astronomy—to meteorology certainly; to natural history perhaps, though here the observations (*metrical*) are also needed, as in astronomy in Newton's time.

How, then, to set to work to improve matters? I have no better plan than yours. Perhaps I should approach the subject from rather a different point of view. I should start with the proposition that the amount of critical discussion (*i. e.*, discussion of any value) of results obtained is likely to depend roughly on the number of men of first-rate ability who can be enlisted into the service. For making observations a moderate ability may suffice, but there is no doubt about the ability required for discussing them and directing future programs. Well, then, I fear it must come to this: That we want more *positions of eminence*—well paid or honored or both—such as the leading professorships. When Schuster gave his address, which you quote with approval, Dr. W. N. Shaw (head of our Meteorological Office) remarked that meteorology had never had any *professorships* at the universities (Is this also true in the United States?), and I think the remark went very near to a sufficient explanation of the lack of adequate discussion of results. You can get heaps of people to measure rainfall, but who is to think about the results? It is more thinkers we want.

Hence my proposition comes to this: Either—

(1) Endow more really first-class posts, such as will attract good men. It is no use getting youngsters into the science unless there is some prospect for them; or,

(2) Look about for means for drawing into the work of discussion occupants of existing positions of repute who are now either wasting their time accumulating little-needed observations or are prevented from doing such work by the lack of machinery—*i. e.*, of funds for getting computing done—for there is a good deal of computing attached to most discussion of masses of observations.

One could accordingly meet the present need in a variety of ways. When you were over here I was speaking of a "calculating bureau" (and you seemed to approve). This would follow from the second part of No. 2. If a man (like Sampson or Durham) knew that he could get computing done pretty easily if he would arrange the details, he might be rendered efficient when otherwise his way would be blocked. The relief might be compared to that afforded in the matter of *printing and publication* which our societies have afforded and which the American observatories are finding in their "bulletins" and "circulars." Before printing was easy much good work must have been lost.

But this is only one way of meeting the need and is practically included in your method, which includes, indeed (if I understand you rightly), all the elements I have sketched. At the head of your suggested organization you could scarcely fail to have at least *one first-rate man*, which so far meets my point 1. You virtually meet the first part of 2 by establishing, instead of a new observatory to multiply observations, an organization of a new kind, which will set a good example to others, and the rest of 2 I have already considered. I have written truly my thoughts as they occur, and hope this letter is not too long and rambling. One can not help, when these inspiring letters talking of new projects come from over the water, building a few castles in the air. One of my castles is a really critical astronomical journal, for discussing the work of others rather than publishing our own. To some extent the V. J. S. does this, but we could do with an English journal of the kind, and a better one. If you get your way perhaps this journal might be tacked on to the scheme.

H. H. Turner.

[*Letter of Karl Pearson.*]

UNIVERSITY COLLEGE, LONDON, ENGLAND,
*June 24, 1904.*

DEAR SIR: I have put together a few suggestions that occur to me, principally based on my own personal experience; but I do not wish them to be considered in any way as dogmatic statements, only as impressions.

(1) I agree absolutely with Professor Newcomb's first statement that the nineteenth century has industriously piled together a vast mass of astronomical, physical, and biological data, and that very little use has hitherto been made of this material. The reason for this I take to be that a man of mediocre ability can observe and collect facts, but that it takes the exceptional man of great logical power and control of method to draw legitimate conclusions from them.

(2) Differing probably from Professor Newcomb, I hold that at least 50 per cent of the observations made and the data collected are worthless, and no man, however able, could deduce any result from them at all. In engineers' language we need to "scrap" about 50 per cent of the products of nineteenth century science. The scientific journals teem with papers which are of no real value at all. They record observations which can not be made of service by any one, however able, because they have not been undertaken with a due regard to the safeguards which a man takes who makes observations with the view of testing a theory of his own. In other cases the collector or observer is hopelessly ignorant of the conditions under which alone accurate work can be done. He "piles up" observations and data because he sees other men doing it and because that is supposed to be scientific research.

(3) I have had to deal to a great extent with the observations and data of other men in my statistical laboratory, to which applications are always being made for aid in the interpretation of observations. I think I might help to illustrate my point by citing a few actual experiences.

(*a*) *Meteorological Statistics.*—We have here a large work in progress. The data are enormous, but without any system. Examination shows that in Europe and America the returns are often untrustworthy. There is no standardization of method, of time, or of quantity observed. Important stations are omitted or dropped for years, and where a well-organized plan for a quarter of the expense and labor would have led to definite results, the existing chaotic

mass of data will only provide probabilities and suggestions. Any man with ideas on the subject of meteorology would after a little experience discard existing material and start afresh, or else waste his best years in trying to reduce material to a common measure, which is really a hopeless task.

(*b*) *Medical Statistics.*—These are made by each medical man and each hospital on a separate plan, and without any idea, as a rule, of the points which it is needful to observe in order that logical conclusions may be drawn. This is especially the case in inheritance of disease tendencies. Further, immense masses of material are wasted because one or other essential factor has escaped record in one or other series.

We have had to report recently on cancer statistics, lunacy statistics, and inoculation for enteric fever statistics. Only moderately definite conclusions can be drawn, because the material has usually been collected without insight into the conditions requisite for drawing definite statistical results.

(*c*) *Physical Measurements.*—The same applies here, in perhaps a less degree, but still quite definitely. Observations on the strength of materials exist in immense quantities. These are largely of no value because the experimenters have had no clear idea *a priori* of the points they wanted to elucidate. Further, this applies to a whole mass of physical observations which have been made without sufficient mathematical knowledge to realize the difficulties of the problem. The failure on this account of physicists like Wertheim, Savant, and Kupffer in the first half of the nineteenth century is quite paralleled in recent work by men whom for obvious reasons it is better to leave unnamed.

(*d*) *Biological and Sociological Observations.*—These are of the lowest grade of value in too many cases. Even where the observers have begun to realize that exact science is creeping into the biological and sociological fields they have not understood that a thorough training in the new methods was an essential preliminary for effective work, even for the collection of material. They have rushed to measure or count any living form they could hit on without having planned *ab initio* the conceptions and ideas that their observations were intended to illustrate. I doubt whether even a small proportion of the biometric data being accumulated in Europe and America could by any amount of ingenuity be made to provide valuable results, and the man capable of making it yield them would be better employed in collecting and reducing his own material.

It will be seen from the above results that I personally can not form a very high expectation of the amount of results of first-class value which would be obtainable by forming an institute to deal with the existing masses of observations.

(4) Nevertheless, if we reject 50 per cent of existing observations as worthless, if we frankly "scrap" them, I still think something of service might be done with the remainder under certain conditions.

(*a*) If the right man were available. This is the chief difficulty. He must be a man of wide appreciation of many branches of science, otherwise a special man will be wanted for each branch—astronomy, meteorology, physics, medical science, sociology, etc. Even were the money forthcoming for this multiplicity of workers, I doubt whether the men themselves are to be found. If Professor Newcomb's institute is carried out, the right man for director will be a man of very exceptional attainments, falling little short of scientific genius. I doubt if one man of this type could be procured. It is certain that several could not.

(*b*) The right man must have been rightly trained. He is to be occupied in drawing logical conclusions from other persons' observations and data. He must therefore in the first place be an adept in scientific method; he must be a first-class mathematician, statistician, and a trained calculator and computator.

(*c*) The right man must be rightly supported. He must have a competent staff of workers under him, and be to a considerable extent a man of affairs. He will have to reject after examination whole masses of observations and data as unsuitable, and his proceedings will be questioned and criticised. Unless he is a man of weight and tact, he will soon be in an impossible position relative to the mediocre observers whose data he is to manipulate. For example, he proposes to deal with the weights of the human viscera in health and disease. He collects all the available data, but issues his report and conclusions, silently passing by the measurements of some well-known physician or hospital, because they have been made in a manner which renders them of no real scientific value. The result would be certainly controversy, possibly uproar, and the director of the institute would have to fight a series of pitched battles before his reputation as a censor and official "scrapper" was finally established beyond dispute. He might survive this initial state of affairs if he had the support of the best scientific minds in the country; but unless he was a strong man he would take the easier course, and simply add another long series of reports on *all*

existing material to the already overvoluminous scientific literature of the day. The right man will be the man who has the courage to "scrap" and to do it relentlessly. Science wants immensely the courageous pruner to-day; but his is not an enviable task, and the Carnegie Trustees would have to support their man pretty steadily to enable him to be effectual. He will be sure to make some mistakes, and these will be at once seized on and trumpeted abroad. If we suppose that the above three conditions can be fulfilled, may we not question whether the man pictured would not be of such caliber that he would do far better work for science if he were allowed to use other people's observations where he chose, and to observe and collect himself where he found them defective or incapable of throwing light on the branches of science he was peculiarly interested in? In other words, the director would be reduced to an ordinary scientific worker, placed in one sense under very favorable conditions, in another under unfavorable conditions; he would have ample material and support, but he would differ from an academic teacher in having no school wherein he might train his subordinates in his methods.

(5) On the whole, I doubt whether the founding of an institute to "scrap" and codify existing observations and scientific material is feasible if desirable. I am inclined to think that more might be done by a *Statistical and Computating Institute*. This institute should have a competent director and a highly trained staff. It should be prepared to report on any data or material submitted to it at a moderate fee. This fee might be remitted on the recommendation of the director, or a committee, in the case of first-class work from a man of scientific repute but small means. It would have to be retained, however, to prevent a flood of worthless material being sent in to be reduced. The institute might also offer advice on the collection of material on observational method and on statistical treatment, again charging a slight fee to prevent the institute being used as a source for providing research work for those who were too idle or too dull to discover such work for themselves. Besides, private individuals, learned societies—astronomical, metéorological, or biological—might and probably soon would use the institute to carry out special investigations on the value of material already amassed in some one or other branch of their special sciences. Finally, Government departments would very soon fall into the habit of asking for reports on the special material of their own spheres. The like course would be taken by local bodies in the case of demographic

and other statistical material. I think that such an institute would be of very great service, and, perhaps as far as possible, fulfill the functions which Professor Newcomb proposes, without the great amount of friction that a direct inquiry into the value of material collected by men, many of whom would still be holding scientific posts, would certainly involve.

Of course one is far too apt to judge matters from one's own little corner of the field of science. We have had a statistical laboratory established for some little time, and we find that an increasing number of workers send us their data for suggestion and report. To such an extent has this become current that we shall probably have either to institute a fee to check the flow of material or else decline to examine such work, as we are only an academic department, doing our own teaching and research work, and without public support of any kind. Still our small experience may be useful on the other side of the Atlantic; and we have found a multiplicity of workers, physical and biological, want assistance, and further that public bodies and government departments seek statistical and calculating aid also. If Professor Newcomb's ideas were carried out first on material which was actually placed before the institute for report, then the action of scientific societies and public bodies would soon give the foundation an established position, from which possibly the more serious business of codifying and "scrapping" existing accumulations of observations and data could ultimately be carried out without too great friction and controversy.

KARL PEARSON.

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[*Letter of Lord Rayleigh.*]

ROYAL INSTITUTION OF GREAT BRITAIN,
*November 20, 1903.*

DEAR PROFESSOR NEWCOMB:

I am in complete sympathy with the views expressed in your letter of October 30, and have indeed sometimes expressed myself in a similar sense; but my experience is far less than yours.

I sincerely hope you may succeed in organizing such an establishment as you indicate.

RAYLEIGH.

[*Letter of G. H. Darwin.*]

NEWNHAM GRANGE, CAMBRIDGE.

I sympathize very warmly with Professor Newcomb's plan for developing the Carnegie Institution and think that it may have a great future. I have been trying to picture to myself how it would work out, and I see that while the gain in some subjects would be great and immediate, in others it would be only collateral.

Scientific observations may be roughly classified in two groups, which, however, graduate into one another. I can best illustrate my meaning by examples.

The subject of the tides seems to belong to the group which would reap immediate advantage. Observations are now published in the most diverse places and are not properly coördinated. A critical collection of tidal results would be a heavy task and would be of much value. There is nothing in this subject which corresponds to probable error in astronomy, for the defects depend on human frailty. It would require a first-rate man to classify and reject observations according to the internal evidence afforded by them. When such a collection was made, generalizations would follow, and the value of the conclusions would probably be great.

Meteorology and many other subjects fall into this group. The distinguishing feature is that we know exactly what to observe, that the mass of material is already enormous, and that it is impossible to have too much matter, provided that it is coördinated.

The second kind of research to which I have referred is intermediate between observation and experiment. The subject of observation is to some degree indeterminate, and it depends on the investigator what he shall observe.

I can not think of a very good example at the moment, but I may perhaps illustrate my meaning by supposing that we were investigating the laws governing the drifting of sand and the formation of sand dunes. It must be obvious that this is a subject of great agricultural importance in many parts of the world. Now, it would be almost useless merely to collect maps and photographs. There must be a guiding mind, forming theories to be proved or disproved by observation. The investigation might be expensive and troublesome, but it is essentially the work of an individual.

In this sort of case I should not look for any great gain from the proposed institution, except that it would afford a fixed position, with good pay, to men of ability. The exception is important, and it

brings us to the point raised by Professor Turner, viz, that the search for men is more difficult and more important than the search for facts.

I hope that you will not regard this long letter as wide of the point, and in conclusion I desire to express my warm approbation of the scheme. G. H. DARWIN.

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[*Letter of Arthur Schuster.*]

KENT HOUSE, VICTORIA PARK, MANCHESTER,
*August 18, 1904.*

In answer to your request to have my views on the letter addressed to you by Professor Simon Newcomb, I will take his various points in order:

I. There can be absolutely no doubt on the correctness of Professor Newcomb's view regarding the piling up of a vast mass of observations, which has been made an object in itself, instead of being a means to an end, and hence a proper discussion has not been able to keep up with the accumulation of undigested figures. The efforts of individuals to discuss results have often been hampered by want of assistance or of funds, and in many cases have been doomed to failure owing to the fact that the men trained to observe are very often not particularly well fitted to draw conclusions. It would be easy to find examples of the waste of labor which has resulted from incompetent work in the planning out of the methods of reduction.

II. Here also I agree with Professor Newcomb, and I would like to add another feature of the present situation which stands in the way of the discussion of great problems on a broad basis—the vast mass of accumulating material has rendered it necessary to have a special journal almost for each special branch of a subject; thus we have a journal dealing with solar physics, and another with terrestrial magnetism, etc.

The mathematician and physicist who is probably most capable of dealing with the problems of solar physics and terrestrial magnetism often never sees these journals. If he does he will get bewildered by the mass of detail which is put before him, and often by technical terms which he does not understand.

What is required here is some intermediate agent whose business it should be, on the one hand, to place before the man of general science the main results of observations which want discussing, and on the other hand before the observer the main facts and measurements which the theoretical student requires for his work.

The efforts which have been made to remedy this recognized difficulty by the publication of abstracts have, in my opinion, proved failures. To write efficiently an abstract which would give the pith of a paper in a form that can be utilized requires a very intimate knowledge of the subject. In a subject requiring special skill and training this can not be expected from those who at present undertake work of this kind, nor is the frame of mind of the reader who takes up one of these journals of abstracts and endeavors to assimilate in half an hour the ideas of one hundred and fifty different workers on one hundred and fifty different subjects such as to make it likely that his thoughts will be usefully fertilized. A much more useful plan would be to have periodical reports dealing with the progress of the subject; but here again all will depend on how far it would be possible to get men who thoroughly understand the subject to write these reports.

It is doubtful to my mind whether the best results ever can be obtained by an observer who has not full grasp of what his observations will be used for; but, dealing with the question from a practical point of view, we must recognize that there are many men who can take excellent observations without any special power of discussing them, and it would be a pity not to make use of such men, provided we can convince them of the limitation of their powers.

III. An institute or bureau of exact science, according to Professor Newcomb's scheme, would, in my opinion, prove useful, as it might in each subject find the best methods of coördinating facts and reducing observations; but the organization of the bureau would have to adapt itself to the different requirements of the different subjects, these requirements probably varying from time to time. In particular stages of a subject publication of a list of papers may be what is required, and in every case we must guard against stereotyping any one particular method of procedure. The abstracts which, as above mentioned, I found useless in my own subject might be very effective in others.

It would be, as Professor Turner points out, a very material gain if there were a body of men whose special duty consisted in discussing observations and drawing attention to those matters where observation is most required. I consider the subjects included in Professor Newcomb's third "field" as requiring most attention at the present moment.

The bureau should, in my opinion, not only have power to initiate reductions, but should also be able to assist other workers in cases

where its council approves of the proposed method. I may mention an example from my own experience. I have engaged during the last two years, at my own expense, an assistant to do certain reductions of sunspot observations by a method which, I believe, will give useful results in many branches of cosmical physics. It would have been advisable in any case that the first set of reductions by this method should have been carried out under my own supervision, but supposing the results arrived at to be valuable and the method to commend itself to competent judges, it would be quite beyond the powers of any individual to extend the calculations so as to include other phenomena, such as prominences or magnetic disturbances, which can be brought into connection with sunspots. The bureau, with funds at its disposal and a committee of directors who could judge of the value of any proposed piece of work, might prevent a block in the advance of science which is at present possible for want of a proper organization.

IV and V. I quite agree that everything must depend on the nomination of a managing head, although an advisory committee will probably be necessary, and it can only be through the organizing powers of a man who is at any rate thoroughly qualified in one branch of science that the work can succeed.

VI. I also agree that the institute should be started on a modest scale. If it is desired that the council should be international, I would suggest that the International Association of Academies should be asked to nominate a certain number of its members. As this association has been founded for the purpose of international coöperation, it seems desirable to strengthen it as far as possible and to avoid the multiplication of other international organizations. I do not, however, wish to express an opinion at present on the desirability of starting the bureau at once on an international basis. It might be better to secure greater elasticity by leaving it, in the first instance, to be an American institution. If desirable, it will always be easy in a few years' time to ask the International Association of Academies to nominate members on its council.

I am sorry there has been so much delay in sending you this reply, but, as I have already informed you, I was unusually busy when your letter reached me.

ARTHUR SCHUSTER.

[*Letter of Edward C. Pickering.*]

HARVARD COLLEGE OBSERVATORY, CAMBRIDGE, MASS.,
*July 27, 1904.*

DEAR SIR: Your letter inclosing a copy of that of Professor Newcomb and requesting a reply before August 1 duly reached me. The plan in general meets with my hearty approval. There is no doubt that a proper discussion of existing observations is very much needed. This should be followed by suitable observations in order to supply the wants thus rendered evident.

To select subjects for the proposed institution a permanent council might be needed, but when a subject was chosen specialists in that department of science should be employed, who would spend several days together arranging the details of the work. According to my experience, a discussion of generalities by a committee with no means at their disposal is unsatisfactory and the results are of little value. A number of experts, however, having an appropriation which they could expend on work with which they were entirely familiar could get much better results than any one person alone. The officer in charge of the proposed institution, with his corps of computers, could readily carry out the plan of work recommended, consulting the committee when difficulties arose, or calling other meetings as required. A large part of the laborious work involved in discussing an extensive series of observations in any department of science could be done to great advantage by such a permanent computing bureau.

It is often impossible to transplant a man of genius in other surroundings without greatly diminishing the value of his work, and it is better to improve his existing conditions rather than try to make him adopt new ones. On the other hand, he is often unable to discuss his own results or supervise large routine computations as well as one who devotes his life to such work. My views on this subject are given more fully in a pamphlet entitled "The Endowment of Astronomical Research, No. 2," which will be distributed in a few days.

EDWARD C. PICKERING.